The two series of experiments are brought into relation by one wire being immersed in hydrogen and the other in water, by which it appears that the cooling effect of the hydrogen nearly equals that of water.

Further experiments are then given, in order to ascertain, if possible, to what chemical or physical peculiarity these cooling effects are due; and from them it appears that they are not due to the specific gravity, specific heat, or to any conducting power of the gases for electricity; and that they do not follow the same law as that by which gases escape from minute apertures. They apparently depend upon some molecular character of the gases, by which either the interchange of hot and cold particles is facilitated, or a superficial action takes place, the surface of the hydrogenous gases presenting a more ready escape to the heat, similarly to that which has been long observed with regard to the different molecular constitutions of solid bodies, such for instance as the more rapid radiation or absorption of heat by black than by white surfaces, in the present case the epipolic action being dependent on the surface of the aëriform medium, and not on that of the solid substances.

December 21, 1848.

The DEAN OF WESTMINSTER, Vice-President, in the Chair.

A paper was in part read, entitled, "Contributions to the Physiology of the Alimentary Canal." By W. Brinton, Esq., M.B. Communicated by R. Bentley Todd, M.D., F.R.S.

The Society then adjourned over the Christmas recess, to meet again on the 11th of January next.

January 11, 1849.

The MARQUIS OF NORTHAMPTON, V.P., in the Chair.

The reading of a paper, entitled, "Contributions to the Physiology of the Alimentary Canal." By W. Brinton, Esq., M.B. Communicated by R. Bentley Todd, M.D., F.R.S., was resumed and concluded.

The paper consists of two parts, having a real relation to each other, though apparently little connected.

I. *On the Movements of the Stomach.*—The anatomy of its muscular coat is first briefly mentioned, and the so-called oblique fibres of some authors stated to be really transverse, *i. e.* at right angles to the altered direction of the canal.

The muscular actions of the digesting stomach are then considered.

These Haller regarded as alternate contractions in two directions, now forwards, now backwards, forcing the contained food in correspondingly reversed directions, and rested this conclusion on experiment and argument; but the author believes the experiment to be solitary, and not parallel with the fact sought to be established, and the argument to be inconclusive.

Beaumont's views are cited as analogous to Haller's, but are considered as having been by no means clearly stated.

The author indicates an argument from analogy, but chiefly bases his conclusion on the observations of Owen and others on Fishes, and his own observations in animals immediately after death:—in the empty or non-digesting stomach; and in the stomach which contains food; first, in the early stage of digestion; and, secondly, at a later period.

From a contrast of these three states it is found, that in the first there is no movement; in the second and third a considerable one; that in the latter, the opening of the pylorus, and the preponderance of the contractions of the pyloric half of the viscus, constitute its chief *distinction* from the second. The two latter movements are both peristaltic, or *in one direction only*—being *never* reversed, so far as the author has seen.

The movement impressed on the food is next considered. According to the observations of Beaumont and others, the food passes in two directions or streams, forwards and backwards. These observations the author has been unable to repeat, but regards them as established.

Assuming the truth of these observations, and contrasting them with the muscular actions previously stated, it appears that the latter are uniformly in *one* direction, the former in *two*,—an apparent incongruity, which the author next seeks to explain.

By experiment he attempts to imitate the natural conditions, and with the production of the like result. He next offers an explanation and illustration of the fact (which might almost be predicated, *à priori*), and adduces some (possible) analogues from the animal kingdom.

He then seeks to establish a general law—that transverse contractions, occurring in a closed tube filled with fluid, and proceeding in *one* direction only, imply *two* currents; a peripheral of advance, taking the same course as the peripheral contractions; and an axial of return, in the opposite direction.

He next points out the modification of this law for stomachs of human shape, and shows how compatible this is with the careful observations of Beaumont, none of which are essentially opposed to it.

The author indicates a probable modification correlative with the *consistence* of the food in some animals, and thus shows a dependence of this physical process on a previous one.

A solitary experiment is adduced to show that, as in the healthy movement, so also in vomiting, no backward or antiperistaltic contraction necessarily occurs.

A conjecture concerning regurgitation of fluid from the stomach concludes this part of the paper.

II. *On the Physiology of Intestinal Obstructions.*—In the preceding part of the paper it has been stated, that two currents probably obtain in the liquid contents of the stomach. Many of the conditions of the intestinal tube approximate to those of the stomach; and if disease or experiment add to these occlusion and distension, the analogy of the two organs is rendered tolerably complete, and the results will hence probably be referrible to the same general principle.

The most remarkable and constant symptom of this state of obstruction is the occurrence of fæcal vomiting.

The author briefly states the theory of an antiperistalsis by which this phenomenon is ordinarily explained: and from an inquiry into its experimental basis he deduces this general result, that an antiperistaltic movement has never yet been observed in any part of the alimentary canal. He regards the irregular actions seen on laying open the bellies of *healthy* animals recently killed, as not definedly peristaltic or the reverse, but as dependent on the irritation produced by the admitted air. So also, in the case of the *occluded* intestine, an inverted movement likewise fails to be recognized. In general, the vermicular actions are more energetic, and more peristaltic, than in the healthy bowel.

He next adduces the following arguments:—

1. The antiperistalsis is usually attributed to irritation; but irritation is present in almost every disease of the tube, while fæcal vomiting is limited to cases of obstruction. This renders it probable that the latter is the cause, and that the process of causation is, like the cause, *physical.*

2. The starting-point of the supposed inverted movement is the fullest part of the bowel, while the place towards which it has set is the emptiest. This condition is inconsistent with the supposition of an antiperistalsis, yet perfectly consistent with a forward movement, and analogous to the obstructions of other tubes conveying fluids.

3. Intus-susception is often the cause of obstruction. But, both from experiment and argument, it appears probable, that an antiperistalsis would at once remove this condition, and would therefore be incompatible with it.

4. The supposed inverted movement is continuous, while the vomiting is occasional. Hence a theory which showed the essential independency of the return of fæcal matters to the stomach, and their ejection thence, would be, so far, preferable.

5. Experiment and observation agree in showing that the ordinary peristalsis obtains immediately below the strangulation. And it is difficult to imagine how or why the same irritation should produce *two opposite* movements in *reversed* directions.

6. The general and comparative date of accession of the vomiting is scarcely compatible with the antiperistaltic theory.

The author next adduces experiments in which the intestine of animals was artificially occluded by a ligature. In exceptional cases,

the ligature sloughed into the canal, and the obstruction was thus destroyed. In all others, the tube was distended *above* the stricture to a variable extent. *Below* the stricture, the intestine was usually empty and contracted for an inch or two. The *contents* of the tube varied both in quality and quantity; uniform fluidity being associated with a large quantity of contents, while their smaller amount was often attended with differences of consistence. The date at which the vomiting acceded varied considerably. In one or two instances this symptom did not occur at all. These differences appeared mainly dependent on—

1. The amount of fluid ingesta,
2. The distance of the stricture from the stomach.

The date of death seemed to vary chiefly with the degree of distension.

He therefore deduces the theory,—That, in an obstructed intestine, a movement of the ordinary (and probably peristaltic) character propels the contents onwards to the seat of occlusion; that a continuance of the process distends, first this part of the tube, and next, those portions above it; that, if the contents are fluid, the ordinary peristalsis tends to develope an axial and reversed current, which returns matter from a lower to a higher point of the intestine;—often from the obstruction to the stomach, whence they are ejected by vomiting.

That in some cases, however, the action is probably much less perfect than this; the consistence of the contents preventing the perfection of these currents throughout the whole course of the tube. But still a mixture results, although a less intimate one.

The author next glances at the mode in which obstruction appears to affect peristalsis, and the nature of the distending fluid. He compares the obstructed intestine to the healthy stomach, to the obstructed artery and duct; referring its peculiar appearances to the dilatable yet muscular structure of its coats.

In conclusion, he indicates the possible result of this theory on practical medicine.

The following papers were also read:—1. "On the Determination of the Difference of Longitude, by means of the Magnetic Telegraph." By Elias Loomis, Esq., in a Letter to Lieut.-Col. Sabine, R.A., For. Sec. R.S. Communicated by Lieut.-Col. Sabine, R.A., For. Sec. R.S.

The writer first refers to a series of experiments made under the direction of Professor Bache, for the determination of the difference of longitude between New York, Philadelphia and Washington, by means of the magnetic telegraph. By this series of experiments he considers it established that, by means of Morse's telegraph, two clocks distant from each other 200 miles, can be compared together with the same precision as if they were placed side by side; and that the difference of longitude of two places can be determined with the same precision as the relative error of the clocks. These results were so satisfactory that Professor Bache determined to pro-